



Mathematics, methods, and modern economics

The Dahlem group on economic modelling

Juselius, Katarina; Colander, David; Föllmer, Hans; Armin, Haas; Kirman, Alan; Lux, Thomas; Sloth, Birgitte

Published in:
Real-World Economics Review

Publication date:
2009

Document version
Early version, also known as pre-print

Citation for published version (APA):
Juselius, K., Colander, D., Föllmer, H., Armin, H., Kirman, A., Lux, T., & Sloth, B. (2009). Mathematics, methods, and modern economics: The Dahlem group on economic modelling. *Real-World Economics Review*, 50, 118-121.

How should the collapse of the world financial system affect economics? - Part III

Editor's note

The Dahlem Group on Economics Modeling and Tony Lawson were invited to respond to the following question.

It is agreed that the current economic crisis has shown that the standard models of academic economics are seriously wanting. Should the main emphasis of reform be on developing new formal models or to an opening up of economics to methods other than traditional modelling?

Neither of the respondents saw the other's contribution before submitting theirs.

Mathematics, Methods, and Modern Economics

The Dahlem Group on Economic Modeling*: David Colander, Hans Foellmer, Armin Haas, Alan Kirman, Katarina Juselius, Brigitte Sloth, Thomas Lux

On the title page of *Foundations of Economic Analysis* (Samuelson, 1947) quotes J. Willard Gibb's famous line "Mathematics is a language." While correct (there is little that is in Samuelson that is not correct), the quotation may also be misleading because it suggests that there is one mathematics. In our view there are many, and it would have been preferable to say that mathematics is many languages.

The key to the appropriate use of mathematics in economics is to find the right mathematics to use for the right problem, and not to claim more for a mathematical model than it delivers. The reality is that in economics there is no perfect mathematics, which means that to appropriately use mathematics and formal models within economics, one must continually be aware of not only the mathematical model, but also the limitations imposed on interpreting the model by the assumptions imposed by the mathematics. Keynes put it nicely:

Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to be this, because, unlike the typical natural science, the material to which it is applied is, in too many respects, not homogeneous through time. The object of a model is to segregate the semi-permanent or relatively constant factors from those which are transitory or fluctuating so as to develop a logical way of thinking about the latter, and of understanding the time sequences to which they give rise in particular cases. Good economists are scarce because the gift for using "vigilant observation" to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one. (Keynes, 1938)

*This group was organized as part of the 98th Dahlem Workshop, "Is there a Mathematics of Social Entities?." The group's specified task was to consider the modeling of financial markets. As part of our duties there we wrote a paper entitled "The Financial Crisis and the Systemic Failure of Academic Economics" which we posted on the web (<http://ideas.repec.org/p/kie/kieliw/1489.html>.) The paper generated significant discussion, including a paper by Tony Lawson to which the editor has asked us to respond. (One of the original members, Michael Goldberg, chose not to participate in this response.)

This view of the use of models within economics follows John Stuart Mill's views (Mill, 1844) that formal models provide us only with half-truths, and we should always keep that in mind both when developing models and when using models to interpret reality.

We are reminded of the above quotations when reading Lawson's critique of our Dahlem Report (Colander et al 2008). In it, Lawson (Lawson, 2009) uses our report as a jumping off point to restate his views on modeling and models. Since he uses our report in that way, and does not directly criticize us, we believe that, after a careful reading, if one interprets the nuances of his statements in a sympathetic way, there is little in the way of disagreement that we have with him, or that he seems to have with our report. In our view, where he seems to disagree, the disagreement results from the way he chooses to interpret our words, by which we mean that he reads into them an interpretation that allows him to make the points he wants to make.

Let us just give one example. Lawson agrees with us that there is concern about the way in which models have been used in macro and in finance, but argues that the problem is with "mathematical deductivist modeling per se." (pg 760) He objects to our alleged support for "developing new formalistic models in their place, ones that are appropriate for our current exceptional times." He quotes our statement as support for that interpretation. Here is the quotation:

The implicit view behind standard equilibrium models is that markets and economies are inherently stable and that they only temporarily get off track. The majority of economists thus failed to warn policy makers about the threatening system crisis and ignored the work of those who did. Ironically, as the crisis has unfolded, economists have had no choice but to abandon their standard models and to produce hand-waving common-sense remedies. Common-sense advice, although useful, is a poor substitute for an underlying model that can provide much-needed guidance for developing policy and regulation. It is not enough to put the existing model to one side, observing that one needs, "exceptional measures for exceptional times". What we need are models capable of envisaging such "exceptional times".

Notice the difference between our statement and the way he interprets it. We call for a model capable of envisaging such "exceptional times" as we have recently experienced. We say nothing about "deductivist modeling." He states that we are looking for a "formalistic" model whereas we only call for an "underlying model." Lawson's replacement of "formalistic" for "underlying," his addition of deductivist, and his removal of our call for a model capable of "envisioning" exceptional times are major changes in our wording and our meaning. That interpretation gives him a springboard into his argument against the use of mathematics in economics, but, in our view, does not engage our argument.

Our Dahlem Report was the result of a five-day intensive discussion, and then much coordination work after that in finding just those words and phrasing that included our many different views. The discussion that led to that statement in our report was a wide ranging one. We considered many different types of models and mathematics—non-linear dynamic models, game theory models, many variations of empirical models, and agent-based computational models. All of them could have varying degrees of formalism associated with them, and could be more or less deductivist in nature. Our report had nothing to say about how formal or what type of modeling should be done, other than whatever model one had should be able to "envision such exceptional times" as the world economy experienced in 2008.

Our primary point was that the macro economy was complex and the underlying model that economists need to have in the back of their minds had to envisage that complexity. This means that the mathematics that we use to deal with it had to be interpreted within the context of that complexity. We felt that some of the standard models were not being so interpreted, and that not to do so was a serious problem. We did not express our views on which mathematics to use or how formal the model should be; those questions were just not issues that we were addressing.

This means that we came to no particular view on the proposition posed to us by the editor.

It is agreed that the current economic crisis has shown that the standard models of academic economics are seriously wanting. Should the main emphasis of reform be on developing new formal models or to an opening up of economics to methods other than traditional modelling?

We do not believe that we should be telling the economics profession what method they should be using. In our view, the main emphasis in choosing a method should be on what works—what adds insight to our understanding. Thus, some economists should be developing formal models, some should be developing agent based modeling, some should be developing heuristic models, some should be developing statistical models, some should be bringing formal models to the data, and some should be trying to integrate the various approaches. In short, researchers should be using whatever approach to understanding that the researcher finds useful.

In doing that research, however, we believe that all economists should be concerned about understanding and developing a model that is capable of envisaging the events that occur in the economy, and should be clear about its limitations as a guide for real-world policy. Put another way we believe that modelers should be modest in their claims for what they have found—carefully spelling out a model's limits, and pointing out inappropriate uses of the model by others. If someone draws implications from a model that cannot be legitimately drawn, the modeler should publicly point that out.

We also believe that researchers should interpret the work of others with a generosity of interpretation that they would want from others. In our view, mathematical models are not the problem; it is the inappropriate use and interpretation of models that is the problem, and the solution lies in being more careful about using and interpreting models, not in any general methodological prescription for or against mathematical modeling. We believe that appropriate modesty of claims and generosity of interpretation will lead to better discussions and advancement of knowledge (what J.M. Clarke (1947) called “communicability”) than will strongly-worded methodological debates.

Bibliography

Clarke, J.M. 1947. “Mathematical Economists and Others” *Econometrica*, April

Colander, D. Hans Foellmer, Armin Hass, Michael Goldberg, Katarina Juselius, Alan Kirman, Thomas Lux, Bridgit Sloth. 2009. “The Financial Crisis and the Systemic Failure of Academic Economics” <http://ideas.repec.org/p/kie/kieliw/1489.html>

Keynes, J.M. 1938. Letter to Roy Harrod. 4, July.

<http://economia.unipv.it/harrod/edition/editionstuff/rfh.346.htm> (Accessed 3-15-09)

Lawson, Tony. 2009. "The Current Economic Crisis: Its Nature and the Course of Academic Economics" *Cambridge Journal of Economics*, 2009, 759-777.

Mill, John Stuart 1838. "Essay on Bentham" in *Mill on Bentham and Coleridge*. ed. F. R. Leavis, Chatto and Windus. 1950.

Samuelson, Paul. 1947. *Foundations of Economic Analysis*. Cambridge: Harvard University Press.

SUGGESTED CITATION:

The Dahlem Group on Economic Modeling, "Mathematics, Methods, and Modern Economics", *real-world economics review*, issue no. 50, 8 September 2009, pp. 118-121,
<http://www.paecon.net/PAEReview/issue50/DahlemGroup50.pdf>